

on May 6, in all 95 fairs which are strictly May 6 fairs; add 56 to the 53 on August 5, and regard them all as strictly August 8 fairs; and add 79 to the 43 on November 8, though there are more fairs in October claiming such recognition.

Our lowest possible estimate of true May-year fairs is now as follows:—

	Feb.	May	Aug.	Nov.	
	77 ...	416 ...	261 ...	321 ...	Total 1074
Astronomical	35 ...	96 ...	109 ...	122 ...	,, 362

The table of the quarter-day groups must likewise be corrected, only to strengthen materially the whole case for the May-year. (Only in a few cases have I been able to correct Owen's spelling of place-names.)



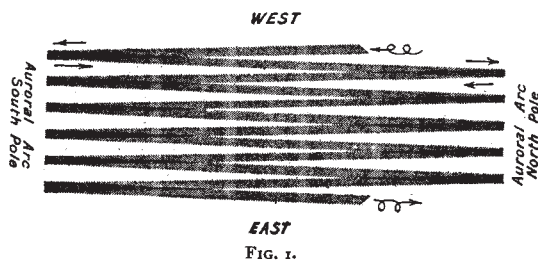
The Bardic Mystic Sign. (Reproduced from "Barddas." "Morien" quotes Payne Knight's "Symbolical Language of Art and Mythology," pp. 69, 70, where it is stated that the same sign, with a small circle or ring at the converging point of the three lines, is a very ancient emblem in Asia Minor. The angles in Knight's sketch, as reproduced by "Morien," are also exactly 28° each.)

George Meredith makes one of his Welsh characters "think in triads." Here is a new triad:—"The three interpreters of the riddle of the stones: the Sun, the Gorsedd, and the Popular Fair." I have already shown (NATURE, May 2) that the May year is the true basis of the Gorsedd. The bardic *Nôd Cyvrin*, Mystic Sign, which Mr. A. L. Lewis (NATURE, June 6) associates very naturally with the "broad arrow," is really a miniature Gorsedd. I have tested several printed cuts of the sign and find the angles to be 28°, that is, regarding the middle line as an east-west line, the right line points to N. 62° E. and the left to S. 62° E., the only possible emblematic representation, in the simplest form, of the May year in the Gorsedd country.

JOHN GRIFFITH.

KATHODE RAYS AND THE AURORA.¹

THE idea that kathode rays play a part in aurora has been advanced by several physicists. Prof. Kr. Birke-land ("Expedition Norvegienne," 1899-1900, Christiania, 1901) has described a number of phenomena produced by kathode rays in the neighbourhood of a magnetised sphere, which resemble various types of aurora. He supposed the sun to be a primary source for kathode rays, which might set up secondary rays in the earth's atmosphere. Mr. C. Størmer has carried out elaborate calculations as to the possible ways in which electrified particles coming from a great distance can approach a magnetised "earth"; his



results tend to limit the approaching particles to the space near the magnetic poles. Prof. S. Arrhenius has supposed electrified particles to be driven from the sun by the repulsion of light and to reach the earth's atmosphere in about two days, originating aurora and magnetic storms.

M. Villard refers to Arrhenius's theory, but seems somewhat sceptical as to the supposed solar origin of the electrified particles. His own views appear to be a combination of theory and observation as to what happens to ions or electrified bodies of any kind moving in a magnetic field. In a uniform field the ion, when travelling with uniform velocity, describes a regular helix on a cylinder the axis of

which is parallel to the lines of magnetic force. The radius of the cylinder varies directly as the component of the velocity perpendicular to the lines of magnetic force, and inversely as the intensity of the field. Suppose, however, that the magnetic field is not uniform, but increases in intensity in the direction in which the ion travels; then, as has been shown by Poincaré, the path forms a curve with diminishing spirals on a cone, and before actually reaching the summit of the cone the particle ceases to advance, and retires, the spirals gradually opening out. If, for instance, the field is that due to two elongated parallel poles, then if the particle gets under weigh between the poles, travelling obliquely to the lines of force, there is a regular game of battledore and shuttlecock, the particle zigzagging to and fro slantwise, reversing its direction whenever it gets within a certain distance of either pole.

M. Villard supposes ions to get in motion somewhere in the earth's atmosphere. As to exactly how this comes about he is not prepared to dogmatise. He is inclined to think that cirrus clouds—which he believes to consist of ice particles negatively electrified—under the influence of ultra-violet light, or less probably under a solar bombardment such as Arrhenius postulates, are probable sources. He also thinks that not improbably a part is played by cosmical dust encountered in the earth's movement through space. Ions starting, say, from a cirrus cloud, and moving obliquely to the lines of magnetic force in the earth's atmosphere, will travel each in a spiral, the whole together forming a sort of luminous spindle, which on getting within a certain distance of, say, the south magnetic pole, turns as if reflected, makes for the north pole, suffers a second reflection there, and so on. Fig. 1, copied from M. Villard's paper, represents the idea diagrammatically. The particles are supposed to come in at the top (answering to the west) and first to travel south. The movement may be supposed to be set up by ultra-violet light from the sun falling on cirrus. The first band or two will thus be in the still illuminated hemisphere, and so invisible; succeeding bands will be overhead in the unlighted hemisphere, and will be visible. Passing further to the east, the energy will be gradually dissipated and the aurora cease to be visible, thus explaining why the late evening, and not the morning, is normally the time of most brilliant aurora.

To fit the theory, the charge carried by the particles must be negative. If it were positive, the motion would be from the east, and the principal aurora would be in the early morning. Fig. 2 reproduces a photograph showing the actual appearance near a magnetic pole from one point of view, in one of M. Villard's experiments. He regards the intensifications of brightness, due to the superposition

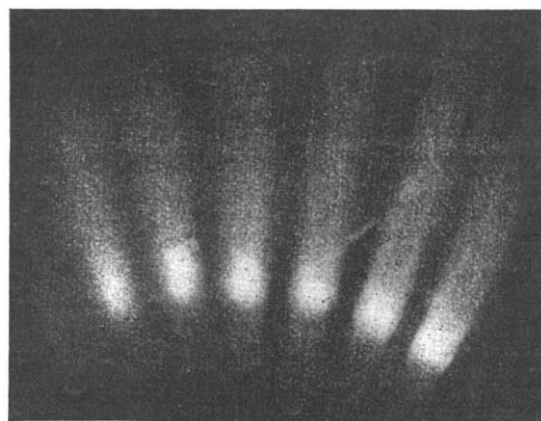


FIG. 2.

of the direct and return paths, as answering to an auroral arc. Below the arc there would, he says, be total darkness—answering to the "dark segment" of the ordinary aurora—but for a special form of discharge which he terms "magneto-kathodic" rays; these rays require, he says, a very steep potential gradient, and do not exist in the earth's atmosphere. Changes in the magnetic field or

¹ "Les Rayons cathodiques et l'Aurore boréale." By M. P. Villard. (Bulletin de la Société d'Encouragement pour l'Industrie nationale, May, 1907.)

in the velocity of the particles alter the distance between the bright patches in Fig. 2, giving the effect of changes of luminosity running round a circle, a well-known auroral phenomenon. Other phenomena analogous to the "dancing" of auroral rays are easily produced by advancing or withdrawing a bar magnet. M. Villard seems to regard magnetic disturbances rather as agents causing auroral phenomena than the converse, but his theory seems still in a somewhat uncrystallised state. Those actually familiar with auroral phenomena will think of a number of points requiring explanation to which M. Villard does not refer; but his researches, like those of Prof. Birkeland, are at all events highly interesting and suggestive.

C. CHREE.

CHEMISTRY AT THE BRITISH ASSOCIATION.

THE proceedings of this year's meeting were largely influenced by the number of foreign visitors, most of whom made lengthy communications to the section. The latter part of the president's address in particular elicited the warmest approval; it was felt that his warning against the danger we now run into of neglecting to cultivate manipulative skill in the young chemist was most timely. The discussions ranged over an unusually wide field; that on valency came first in importance; in its course the new doctrine of Barlow and Pope was subjected to searching criticism, although the general tone was far from hostile. Of deep significance, as illustrating the importance of maintaining the very closest connection between science and industry, was the discussion on the quality "strength" in flour; it was felt by those concerned in the debate that this did much towards establishing clear ideas on this controversial subject.

Prof. Pope opened the discussion on valency with an account of the theory put forward by Mr. W. M. Barlow and himself. Combining chemical with crystallographic data, it is possible to show that the fundamental valency of an element represents, very approximately, the number of units of volume contained in the polyhedral cell which constitutes the domain of its atom in any molecular assemblage. Valency, from this point of view, is primarily a simple volume relation. If the atoms are regarded as centres of attraction and repulsion, a crystalline structure may be considered to be an equilibrium arrangement of such centres. If each atom be allotted its own polyhedral cell or sphere of influence, the simplest assumption that can be made as to the shape of these spaces is that they depart as little as possible from sphericity. The polyhedral cells must fill space without leaving interstices, and the centres of the identical polyhedral cells will be those of the centres of a closest packed assemblage of equal spheres. There are two varieties of homogeneous arrangement of equal spheres in closest packing, distinguished as the cubical and the hexagonal closest packed arrangements.

A crystalline element, in which no molecular aggregation of the single atoms occurs, should thus exhibit holohedral cubic or hexagonal symmetry. Of the forty known crystalline forms of the elements, only six are neither cubic nor hexagonal; in the rest the axial ratios all approximate to the theoretical values calculated on the above hypothesis. The binary compounds can be constructed from spheres of two kinds, but of approximately equal size, present in equal numbers.

This hypothesis has been worked out in detail in the case of the alkali haloid salts and of silver iodide. The assemblages thus constructed present geometrical properties which are entirely in harmony with corresponding physical properties of the crystalline material. The cleavage, twinning, and gliding of the cubic alkali haloid salts and the dimorphism of silver iodide are precisely paralleled by corresponding properties of the assemblages suggested as representing the crystalline structures of these salts.

Each crystalline substance is to be regarded as a close-packed homogeneous assemblage of the spheres of atomic influence.

Prof. Sollas criticised the form of close packing put

forward by Messrs. Barlow and Pope, and objected to the structure suggested for silver iodide on the grounds that it did not explain the sudden contraction in volume of this substance when heated to a certain temperature. He described and showed models to illustrate an alternative system. Prof. Miers agreed that the fact that more than 80 per cent. of the elements and binary compounds crystallise in the cubic system was a strong argument in favour of closest packing, yet this principle must be tested by all the physical properties as well as the geometric form of the crystals. Dr. Tutton also criticised the idea of closest packing; he suggested that the topic axial ratios should be taken as an index and measure of the relative closeness of the packing.

Prof. Abegg pointed out that, according to van der Waals, in the liquid state three-eighths of the volume are occupied by matter and five-eighths are free space, and that as substances changing from liquid to solid do not vary essentially in volume, this would seem opposed to the idea of close packing. In answer to the above, Mr. Barlow explained that the spheres were only in reality centres of influence and attraction, and thus there was no real distinction between loose and tight packing. Prof. Tilden remarked that the authors in their explanation of what happens when a change of valency occurs did not appear to recognise that there are two kinds of valency, the one corresponding to the electric charges and the other an extra valency developed under special conditions.

Prof. Larmor thought that the views of the authors seemed to carry weight in their application to the forms of crystals. The approximation that is exhibited by actual crystalline forms to those calculated for dyad molecules consisting each of two equal spheres or nearly spherical domains of influence seemed noteworthy; it gained even more weight when it was remembered that considerable difference in size of the spheres would produce but little difference in the ratios, so that the restriction to equality of the domain of influence could be largely dispensed with and yet the results remain substantially as they are. He hazarded the suggestion that the direct operations of the chemist can grasp only those molecules which have pronounced architectural features, but that there may be whole regions of incipient combinations which do not submit to architectural modification, though they may be recognisable indirectly, as by the spectroscope.

The electrochemical aspect of valency was to associate it with the number of loose and displaceable electrons in the atom. The essential physical features of a metallic atom being generally that a certain number of its negative electrons are thus relatively free, we may imagine that when it is in combination to form a molecule of a salt these electrons are attracted across into the domain of the radicle thus united with it, and when accidental separation occurs in the appropriate way they may remain there. But while something like this is a fact, the mechanism remains largely a mystery, as it has been ever since valency was first connected in this way with electric displacement by Faraday eighty years ago.

Prof. Werner (Zurich) and Prof. Abegg (Breslau) gave descriptions of their views on the broader questions of valency. Prof. Kauffmann (Stuttgart) spoke on the divisibility of valency, and dealt briefly with his own investigations on the colour and fluorescence of organic compounds in connection with structure.

The second morning was entirely given up to a joint discussion with Section G on explosion temperatures, in which Profs. Boudouard, Haber, Smithells, and H. B. Dixon spoke on the chemical side. In the afternoon Prof. Dixon described his observations on the ignition point of various gases and mixtures. In the discussion which followed Mr. Dugald Clerk dwelt on the trouble arising from pre-ignition in large gas engines, and showed that it was important to be able to predict the ignition temperature from the nature of a mixture of gases, and so to allow proper compression space in designing the engine.

Dr. H. O. Jones gave a concise account of work carried out with Sir James Dewar on iron carbonyls. The pentacarbonyl is a yellow liquid, which resembles nickel tetracarbonyl in its properties, but is more stable towards